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## *Comments on Mr. Dyson's "Note on the Instrumental Errors affecting Observations of the Moon."* By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

(*a*) Mr. Dyson courteously submitted the preceding paper to me in MS., and I offer the following comments upon it which it may be convenient to have in the same number as the paper itself.

(*b*) Paragraphs 1, 2, 3 of his note call for no remark from me.

(*c*) In § 4 our views begin to diverge, but only in the phrase "to obtain from the accordance of the separate determinations a measure of their accuracy." The danger of this test is well known even when all conceivable systematic errors have been removed. When there is an obvious source of systematic error still affecting the observations, I doubt whether the test can be applied. But we shall come across this point more definitely later.

(*d*) On § 5 I would urge that if there really are "no means available for determining the instrumental correction, &c., &c."—that is, if the solar parallax as determined from observations of the Moon is affected with an entirely unknown systematic error, then it is a pity to publish the result. To do so is quite likely to make trouble, from the value being carelessly used irrespective of the limitation on some occasion or another; and that it is so near the probable truth only increases this likelihood. And what good is it? We know that the solar parallax is there or thereabouts; what we want to know now is, if possible, where *exactly* it is.

(*e*) On § 6 I have no comment.

On § 7 I have not yet before me the paper of Mr. Cowell's quoted, but I gather that the column referred to only gives the values of sin D. This only gives information concerning a very small part of the instrumental error for two reasons.

Firstly, if the instrumental error be expressed as a series

$$a_1 \sin D + b_1 \cos D + a_2 \sin 2D + b_2 \cos 2D + \text{ \&c.}$$

the coefficient $b_1$ at any rate is likely to be large. For the instrumental error is almost certain to be quite different at new moon and full moon, *i.e.* at $D = 0°$ and $180°$. Now *sin D* has the same value at these points, and thus terms like $a_1 \sin D$ alone cannot represent errors of this kind. To trace changes in instrumental errors we must have at any rate the coefficients of cos D as well as those of sin D, and probably the higher coefficients also.

Secondly, *for expressing instrumental error* I do not trust the coefficients found in this way at all, for the reason that we have so few observations near new moon. It must be remembered that the case is totally different from that of the determi-

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nation of a function whose form is known, in which case we can find its coefficient from a single value even. Suppose, for instance, we had two observed functions $f_1$ and $f_2$ thus:

| D = | 0° | 30° | 60° | 90° | 120° | 150° | 180° |
|---|---|---|---|---|---|---|---|
| $f_1$ = | 0 | 1 | 2 | 3 | (2) | (1) | (0) |
| $f_2$ = | 0 | 1 | 2 | 3 | (4) | (5) | (6) |

the first rather like $3 \sin D$ and the second rather like $3(1 - \cos D)$; and suppose that we had no observations beyond $D = 90°$, so that we were in entire ignorance of the values except from 0° to 90°, where the functions coincide. Whatever analytical process we employed to represent them by series of sin D and cos D &c., we should of course get the same result for $f_1$ as for $f_2$; and yet the accordance would be quite illusory. This is only exaggerating for illustration what Mr. Cowell has done, if I understand him rightly. Observations near full moon where the instrumental errors are probably nearly constant (because the conditions remain nearly the same) receive most weight and largely control the analytical results. Thus, to adapt the above illustration a little nearer to the facts, suppose we had two quite different instrumental errors

| D = | 0° | 30° | 60° | 90° | 120° | 150° | 180° |
|---|---|---|---|---|---|---|---|
| $f_1$ = | 3 | 3 | 2 | 1 | 0 | (−1) | (−2) |
| $f_2$ = | 3 | 3 | 4 | 5 | 6 | (7) | (8) |
| Weight = | 10 | 10 | 6 | 2 | 1 | 0 | 0 |

The great weight (of course purposely exaggerated) assigned to the observations near 0° would give nearly similar coefficients for the periodic terms sin D and cos D &c. and thus entirely mask the real and serious instrumental difference.

(*f*) With § 8 I entirely agree. Perhaps the large accidental errors of the altazimuth observations were scarcely sufficiently emphasised in my paper.

(*g*) With regard to § 9, as also to § 4 preceding, it seems to me very important to exclude any idea that the length of the series of observations discussed can improve the value of the solar parallax thus obtained. I do not see how a possible systematic error can be in any way diminished by multiplying observations, unless it is equally likely to occur with positive and negative signs; and in the case before us this is unlikely. At any rate the *onus probandi* is on those who make this assumption. If it is thoroughly understood that the value of the solar parallax is put forward, not as an exact value, but as one affected with an unknown systematic error, my objection is so far met, though I think it a pity to multiply imperfect values.

(*h*) But I will say in conclusion that I do not feel sure that Mr. Dyson and Mr. Cowell are right to despair of determining

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this error, or at least of narrowing its limits. The altazimuth observations could be cleared of the error for high and low moon, with some trouble perhaps, but still it could be done, since observations in the same phase but at different altitudes could be compared. And then they could be used to throw some light on the errors of the meridian instruments. This would be one way. Another way would be to try the effect of restricting the determination of sin D to the days near full moon, where the instrumental or observational errors may be supposed with more legitimacy to remain constant, and where sin D is nevertheless varying rapidly. This would be easy to do, and a comparison of the results with those previously obtained from the whole material would perhaps tell us something valuable about the limits of error.

P.S.—After the above had been sent to the printers the following note was received from Professor Newcomb. In an accompanying letter he leaves open the question of printing it; but as it contains at least one important correction to what has already been said, there can be no doubt of the propriety of publishing it here. I refer to the question of the *division errors* of the azimuth circle of the altazimuth, to which Professor Newcomb draws attention. For my own part I was quite under the impression that these had been applied, so that Professor Newcomb's remark would have lost point; but on submitting this opinion to Mr. Dyson he replied (1904 May 20):

> "No division errors were applied before 1880, when the circle was redivided. This I have verified from the computing books. A determination was made in 1847, but apparently never used. I don't know how good it is."

Hence some preliminary work, which I had overlooked, is apparently necessary before the altazimuth observations can be used to discuss systematic errors.

On another point a word may be added to Professor Newcomb's note. The observations of altitude are not *all* made on the lower limb; in the second and third quarters the upper limb was sometimes observed near rising and setting respectively, as may be seen in the printed volumes. Whether there is enough material to determine the error of diameter from these I can scarcely say. Further, as I have remarked in a footnote on p. 405 of this volume, the combination of the altitude observations with those of azimuth may reduce the effect on the longitude in all cases to a simple error of semi-diameter.

Finally, I am glad to have confirmed by such an authority the view expressed in the last sentences of my remarks above—that in Professor Newcomb's words "the best result of meridian observations is to be obtained by throwing out all the observations made in daylight and eliminating the correction to the semi-diameter from the equations themselves." This experiment would in any case be most interesting.

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## *Remarks on the Determination of the Parallactic Inequality of the Moon.* By Simon Newcomb.

The following remarks are suggested by the papers of Professor Turner and Mr. Cowell in the December and March numbers of the *Monthly Notices* (vol. lxiv., Nos. 2 and 5). They relate to sources of possible systematic differences between the results of meridian and altazimuth observations of the Moon and possible systematic errors in a determination of the parallactic inequality which are not fully considered in the papers referred to.

Beginning with the meridian observations, it is necessary, in order to have a determination of the parallactic inequality free from systematic error, that all the observations used should be free from such systematic errors as are a function of the age of the Moon. The practical semi-diameter of the Moon, as it enters into the meridian observations, should not be regarded as a constant. Saying nothing of personal equation, which we assume to be eliminated from the mean during any half-lunation, the practical semi-diameter of the Moon is the true semi-diameter augmented by radiation. The latter is a function of the contrast between the brightness of the Moon's limb and that of the sky. It is therefore greater after the end of twilight and least when the Sun is above the horizon. It will also be less at the full moon on account of the greater obliquity at which the rays strike, and from the fact that the illumination of the extreme limb will be deficient, owing to many points on it being thrown into shadow when the observer is not practically in line with the Sun. For this reason we must expect the semi-diameter determined from transits to be too small. Under these circumstances it seems to me the best result of meridian observations is to be obtained by throwing out all observations made during daylight, and eliminating the correction to the semi-diameter from the equations themselves. I am inclined to think that we should get the best results if the discussions were confined to observations between November 1 and February 15. Each observation would give an equation of condition between the corrections to the semi-diameter and to the parallactic inequality.

The altazimuth observations are liable to errors, which are a function of the azimuth, unless the errors of graduation are determined and the instrument is so constructed as to be perfectly free from wabble. Observations with this instrument near and before the time of first quarter will commonly be made in the south-west or west, while those near the third quarter will be made in the south-east or east. We must also recognise the possibility of systematic errors in altitude as well as those of semi-diameter. A little consideration will show the following results of these errors:—

1. A constant error in altitude will have opposite effects